

dence in structural characters as guides to the recognition or separation of species. Structure and specific nature have grown as one, united from the start, and should some deviation have arisen, with growth this can only increase and the two become ever further divided. Two structural forms cannot unite again into one, growth can only increase the difference.

## *Pieris* Specimens for Androconia: the end of the "Hybrid Species"?

By S. R. BOWDEN

Warren (1971) has given an account of events which led him to reject bred specimens for use in his androconial work, and to advise others to do likewise. It will, however, be necessary to go further back, to understand what happened.

I never quite followed Mr Warren in his association of deformed scales with hybridity. I wrote to him (in 1966) that the *bryoniae* × *oleracea* specimens had shown that inter-specific hybrids need not have any deformed scales, so that the precise cause of deformation (when it occurred) was rather obscure. I was at that time unable to begin any systematic study of androconia myself, because although the procedures are simple they do consume some time, which was lacking. Nevertheless, it would have saved us all a great deal of trouble if I had pursued the matter then, and I am sorry that I did not.

The precise criteria by which hybrid-type scales are to be recognised have not been entirely clear. Basal prongs tending to an hour-glass shape, combined generally with asymmetry of the whole scale (Warren 1966: figs. 5,9) would appear to be the most characteristic manifestation, others being subject to intuitive interpretation.

In 1970-71 one of my first tasks was to show that other factors besides hybridity could produce scales distorted in this way. I looked at specimens from broods including also partial cripples, and found what I expected. Some of these were hybrids, and some were of subspecies that Warren graded as "hybrid species." To prove my point, I had to find deformed scales in undoubted pure species. This I was able to do most easily by looking at long-retarded "spring" emergences of English *napi*, Lapland *adalwinda* and Swiss *bryoniae*. Although the slides that I then sent to Mr Warren showed many grossly abnormal scales, it was generally possible to find normal ones predominating in brother butterflies.

Perhaps the worst conditions for the development of adult *Pieris* are provided by post-diapause temperatures fluctuating just above and just below the minimum required for imaginal development to continue (ca 6°C?). Unfortunately refrigerating systems have their de-frosting periods and even temporary failures. Sharp cooling of pupae after the initiation of development can be very deleterious (Bowden 1955). It is possible

that no abnormalities would be produced even by retardation exceeding six months if the breeder could ensure a temperature continuously near 0°C.

"Summer" (non-diapause) individuals are of course not retarded by the breeder and (unless very late in the season) in nature too suffer fewer development-disturbances than the "spring" butterflies. There is no obvious reason to prefer wild to bred specimens of this emergence, since the difference is likely to be no greater than between two wild samples. One cannot, unfortunately, altogether confine oneself to these "S" individuals, because some populations produce none, and the single brood is certainly not the equivalent of the summer brood, though it might sometimes be convenient if it were.

Many hybrids suffer diapause disturbances, leading in some cases to delayed emergence or to the death of one sex before eclosion (Bowden and Easton 1955). In these cases any scale-distortion really is (indirectly) due to hybridity, and if the hybridization took place in the wild distortion would still presumably occur. But such distortion would not suffice as evidence of hybridity. Even in the wild, pupae are often subject to irregular temperature regimes.

Something of this kind may have affected wild-caught specimens on which Warren has erected his "hybrid species." I do not think it necessary to suppose that anything very unusual should have happened to them: the normal variation between adjacent androconia shows that they are sensitive to the slightest of local influences.

Mr Warren says (1971) that he has had specimens from me that gave perfectly developed scales, and he suggests that these were from eggs of wild females. In nearly all cases, I think, they were not. There is no foundation for the supposition that continued captive breeding alters what he calls "specific nature". Of course the recessive lethals and sub-lethals present in most wild *Pieris* stocks cause trouble if continued *in-breeding* is practised.

Warren's identification of the Corsican and certain other "*Pieris napi*" populations as "hybrid species" depended on an androconial comparison with hybrids of mine which he now rejects (for this purpose, rightly). In these specimens the presence of "hybrid" scales was never a direct consequence of hybridity, but of disturbance which could have had many origins, e.g. interrupted imaginal development. Nothing remains to associate any scale abnormality unequivocally with hybrid origin. Warren has examined (1970) some supposed wider hybrids of natural origin and found these to have scales free from deformity, so he cannot be surprised that his criterion has proved illusory. For it would be odd if relatively close hybrids were more severely disturbed than those between remote taxa.

So all the "hybrid species" revert to normal subspecies, which in any particular case may or may not deserve specific rank. In the words of T. H. Huxley, among the greatest tra-

gedies of science is the slaying of beautiful hypotheses by ugly facts.

I must strongly advise those studying androconia for taxonomic purposes to disregard all deformed scales entirely; when many are present on a specimen it will be safest to transfer attention to another individual.

I am still convinced that Pierine androconia, difficult as they may be to interpret, are of high value in the study of relationships. We must be grateful to Mr Warren for taking these attractive structures and squeezing them very hard indeed to get the last drop of information out of them.

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## Butterflies in Arctic Scandinavia, 1971

By M. R. SHAW

It has frequently been emphasised in the various entomological journals that collecting butterflies in the European Arctic is always something of a gamble. While it is potentially an immensely rich region, the onset of the entomologically brief summer season regularly varies by as much as two or three weeks from year to year, and in a manner which seems impossible to predict at a usefully early date. Also the weather is so notoriously unstable that several unfortunate would-be collectors making relatively brief trips have had to come back without seeing the sun. This year, my wife and I were able to take a complete month's holiday, which we felt would best be used in fulfilling a long-standing ambition to collect in the Arctic. With a whole month we were sure we would see something of at least the commoner butterflies, and we tried to arrange our dates so as to arrive in time for the early flying species, yet still be there when the later species started to emerge. From a survey of the literature it seemed to me that Abisko was on the whole a little later than the northern Norwegian fjords, and thus it should be possible to arrive at Abisko and collect "early" species for a while, then to move northwards towards the remains of the Gulf Stream at lower altitudes and find the "late" species flying perhaps a couple of weeks earlier than they would at Abisko.